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With an emphasis on the problems of control of extraneous variables and threats to internal and external validity, the arrangement or design of experiments is discussed. The purpose of experimentation in an educational institution, and the principles governing true experimentation (randomization, replication, and control) are presented, as are three pre-experimental, three true experimental, and ten quasi-experimental designs with the advantages, disadvantages, and appropirate statistical treatments for each. This discussion follows Campbell and Stanley's chapter on experimental design in Gage's "Handbook of Research on Teaching". (MC)

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EXPERIMENTAL AND QUASI-EXPERIMENTAL DESIGN

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Experimental Design

Experimental research design has as its purpose the establishment of causal relationships between variables. It implies control of independent variables (experimental group -vs- control group). An attempt is made to generalize from experience to law/s or principles. Effective experimentation requires careful planning.

Before beginning to collect data for an experimental study, one should be able to have completely written-up the study to the conclusion, stating criteria for measuring the dependent variable, controls, data format, hypothesis/es to be tested, including statistics to be used.

Purpose of Experimentation

As stated above, experimental research seeks quantitative formulation of verifiable general laws and its ultimate aim is establishment of a system of concepts and relations (the so-called nomothetic net) in which all specific propositions are deducible from a few general principles. This is "basic research."

Most research in education, according to Ebel,[1] should be directed toward studies designed to yield information immediately useful in the solution of contemporary educational problems. This is usually termed "applied research" - collection of data (experience) that promise help in the solution of some immediate practical problem. Applied research can still be quite carefully designed and controlled, involving a good deal of design and statistical sophistication.

Institutional research can be basic or applied, but most often it is applied research.

Principles Governing True Experimentation

Achievement of the three following principles is "something else."

1. RANDOMIZATION - in which it is left to chance, or random methods, the determination of which subjects receive which treatment. There will undoubtedly be some factors over which we have little or no control. This is the chief principle underlying experimentation.

2. REPLICATION - in which we must be able to repeat or reproduce the experiment with a number of subjects and in a number of different settings (internal and external validity).

3. CONTROL - in which the experimenter controls or manipulates what happens to whom, when and where. It is possible to build into the study a comparison group which is similar to the experimental group except that it did not receive experimental treatment.

Since we are not always able to achieve these "ideal" principles, we will later need to look at some Quasi-experimental Designs. In true experimental design we need to consider eight factors which may jeopardize internal validity and produce effects on our subjects confounded with the effect of the experimental stimulus or treatment, and four factors which may jeopardize external validity or the generalizability of the findings beyond the local population or sample of our study.

Principles of True Experimental Design[2]

A. Factors which may jeopardize internal validity

1. HISTORY - specific events beyond the experimental variable treatment which occur between critical measurements and over which the experimenter has little or no control. (eg - Between pre-test and post-test, in an experiment on effect of a reading acceleration machine on reading rate, and the day before retesting, a

major event occurs, such as a paralyzing winter storm or the president is assassinated)

2. MATURATION - processes <u>within</u> the experimental subjects which operate as a function of passage of time per se. (Eg - growing older, getting hungrier, getting more tired, etc.)

3. TESTING EFFECT - factors which affect or threaten internal validity due to repetition of the same test, or of another form of the same test, and will tend to produce some change in results simply due to test-retest situation alone.

4. INSTRUMENTATION EFFECT - changes in calibration of a measuring instrument OR, more usually, changes in the "observer's" or "rater's" judgment.

5. STATISTICAL REGRESSION - when experimental subjects have been chosen on the basis of their <u>extreme</u> scores on a measurement instrument, invalidation of the experiment is likely to occur, since scores on retest of this group will tend to regress toward the mean of that larger group from which the extreme group was selected.

   The only instance when this would <u>not</u> be true would be if the test had perfect reliability (none of this type has <u>yet</u> been developed!)

6. SELECTION - biases introduced due to method of selection may seriously threaten internal validity. (Eg - If randomness in selection of experimental and control groups is adhered to, selection biases are usually eliminated, especially if a sample of sufficient size is drawn)

7. EXPERIMENTAL MORTALITY ("the bane of all experimenters") - the differential loss of subjects in the experimental and control groups during the process of the experiment.

8. SELECTION-MATURATION INTERACTION - selection of subjects, though random, may produce interaction with other factors, usually maturation, confounding the effect of the experimental treatment. (Eg - A study of the "Effect of An Experimental Discussion Program Upon Realism of Vocational Choice." A group of sophomores is randomly selected and the realism of their vocational choices at end of experimental program is compared with realism of vocational choices of a random group of freshmen. Selection of more mature sophomores to compare with less mature freshmen has introduced maturation as a threat to internal validity.

Comparison of two randomly selected groups of sophomores, one of which received the experimental discussion program, would have been more desirable.

B. Factors which may jeopardize external validity

9. THE REACTIVE OR INTERACTIVE EFFECT OF TESTING, AS SUCH - the act of pretesting experimental and control groups may affect their reaction or responses, increasing or decreasing subject's sensitivity, thus making results not generalizable to the unpretested population as a whole.

10. INTERACTION EFFECTS OF SELECTION BIASES AND THE EXPERIMENTAL VARIABLE/S - (Eg - The effect of a course, "History of Religions", upon tolerance in a small rural school might show a different correlation with growth in tolerance than the same course might show in a large urban or suburban school.)

11. REACTIVE EFFECTS OF EXPERIMENTAL ARRANGEMENTS - the reaction of subjects to knowing they are "in an experiment" and the objects of special attention may produce effects which confound experimental treatment.

(The famous Hawthorne Effect is an example of this, in which women workers in both the experimental and control groups showed significant increases in production just as a result of being shown special attention)

12. MULTIPLE-TREATMENT INTERFERENCE - whenever multiple treatments are applied to the same subjects, the effects of the prior treatments may not be completely erasable. (Eg - A study of the effect of classroom climate upon learning might subject one randomly selected group to an authoritarian climate the first 6-week period, another to a "democratic" climate, and a third to a "laissez-faire" climate. The second 6-week period, the first teacher would "switch" to a democratic climate, the second to laissez-faire, and the third to authoritarian, and so on. The effects of authoritarian climate upon the first group could not possibly be kept from confounding their subsequent experience with later democratic and laissez-faire treatments.)

Three Pre-experiental Designs[3]

Design 1. The One-Shot Case Study - Much research in education conforms to a design in which a single group is studied only once, subsequent to some agent or treatment presumed to cause change. Such studies might be diagrammed as follows:

X	O_1
(Treatment)	(Observation or measurement of effect)

Such studies have such a total absence of control as to be of almost no scientific value. Careful, elaborate measurement and use of detailed description and statistics in such studies represent "The Error of Misplaced Precision."

Design 2. The One-Group Pretest-Posttest Design -

$$O \quad X \quad O_2$$

Reference to Campbell and Stanley will present the many weaknesses and the slight increase in internal validity of this design over Design 1 above. Some control is gained in the Selection and Mortality factors. It is still considered "bad design" for educational research.

Design 3. The Static-Group Comparison Design - This is a design in which a group which has experienced X is compared with a group which has not, for the purpose of establishing the effect of X.

$$\begin{array}{ll} X & O_1 \\ \text{----------} & \text{----} \\ & O_2 \end{array}$$

This design controls a few more of the factors (threats to internal validity), but now does not control for Selection and Differential Mortality between the two groups.

Three True Experimental Designs[4]

Design 4. The Pretest-Posttest Control Group Design -

$$R \quad O_1 \quad X \quad O_2$$

$$R \quad O_3 \qquad O_4$$

(Infers random selection)

This design, if carefully followed, neatly controls at least seven of the eight internal validity threats (See TABLE 1 in the paperback, p. 8).

Design 5. The Solomon 4-Group Design -

R_1 (Group 1, exper.)	O_1	X	O_2
R_2 (Group 2, Control)	O_3		O_4
R_3 (Group 3, Exper.)		X	O_5
R_4 (Group 4, Control)			O_6

This design parallels the Design 4 groups with two additional randomly selected groups (experimental & control) which lack the pretest. Therefore, both the main effects of <u>testing</u> and the interaction of <u>testing</u> and X are determinable.

In this way, not only is the generalizability increased, but in addition, the effect of X is replicated in four different fashions: $O_2 > O_1$, $O_2 > O_4$, $O_5 > O_6$, and $O_5 > O_3$.

Design 5 lends itself to use of the 2 X 2 analysis of variance design in treating the posttest scores.

	No X	X
Pretested	O_4	O_2
Unpretested	O_6	O_5

If the main and interactive effects of pretesting are negligible, it may be desirable to perform an analysis of covariance of O_4 versus O_2, pretest scores being the covariate.

Design 6. The Posttest-Only Control Group Design

R	X	O_1
R		O_2

"The pretest is not actually essential to true experimental designs," say Campbell and Stanley. They go on to say, "For

psychological reasons it is difficult to give up 'knowing for sure' that the experimental and control groups were 'equal' before the differential experimental treatment. Nonetheless, the most adequate all-purpose assurance of lack of initial biases between groups is randomization."[5]

Design 6 is appropriate to all of the settings in which Designs 4 or 5 might be used, i.e., designs where true randomization is possible. A very lucid account of the advantages and disadvantages of Design 6 over Designs 4 and 5 is given by Campbell and Stanley here.

The t test is the simplest form of statistical treatment for Design 6, the only setting for which this test is optional. Covariance analysis and blocking on "subject variables" (B. J. Underwood, Psychological Research. New York: Appleton-Century-Crofts, 1957) such as prior grades, test scores, parental occupation, etc., can be used, thus providing an increase in the power of the significance test very similar to that provided by a pretest.

Quasi-Experimental Designs[6]

In situations occurring in many natural social settings, where full experimental control is lacking, the researcher can still introduce something like experimental design, even though he cannot have full control over scheduling of experimental stimuli and of randomization. He still has some measure of control over when and to whom measurement data can be recorded on. It becomes imperative, then, that the researcher be fully aware of which specific variables his particular design fails to control.

Please refer to the introductory paragraphs on quasi-experimental designs[7] and the theory of experimentation for a lucid overview of the place of experimentation in controlled laboratory situations and in the field.

The inherent strengths and weaknesses of the following list of quasi-experimental designs are given in Campbell and Stanley's chapter of Gage's Handbook... (and the paperback copy of this chapter). These cannot be covered in this brochure in brief or cogent fashion.

Design 7. The Time-Series Experiment

O_1 O_2 O_3 O_4 X O_5 O_6 O_7 O_8

Design 8. The Equivalent Time-Samples Design

X_1O X_0O X_1O X_0O etc.

Design 9. The Equivalent Materials Design

M_aX_1O M_bX_0O M_cX_1O M_dX_0O etc.

Design 10. The Nonequivalent Control Group Design

O X O
O O

Design 11. Counterbalanced Designs

	Time 1	Time 2	Time 3	Time 4
Group A	X_1O	X_2O	X_3O	X_4O
Group B	X_2O	X_4O	X_1O	X_3O
Group C	X_3O	X_1O	X_4O	X_2O
Group D	X_4O	X_3O	X_2O	X_1O

Design 12. The Separate-Sample Pretest-Posttest Design

R O (X)

R X O

Design 13. The Separate-Sample Pretest-Posttest Control Group Design

R O (X)

R X O

R O

R O

Design 14. The Multiple Time-Series Design

O O O O X O O O O

O O O O O O O O

Design 15. The Recurrent Institutional Cycle ("Patched-Up") Design

Class A X O_1

Class B O_2 X O_3

Design 16. Regression-Discontinuity Analysis

This quasi-experimental design attempts to substitute for the true experimental Design 6 by examining the regression line between independent variable (such as ability test scores) and dependent variable (eg. - later achievement) for individuals in a narrow independent variable score interval (i.e. - with essentially equalability scores) at or slightly below the "cut off" score above which a certain award or motivational opportunity is given. If discontinuity of the two regression lines exists at the cut-off score, "...the evidence of effect would be quite compelling, almost as compelling as in the case of the true experiment."[8]

Summary

In conclusion, in this brief brochure, we have discussed alternatives in the arrangement or design of experiments, with particular regard to the problems of control of extraneous variables and threats to validity, both internal validity

threats (8) and external validity threats (4).

Sixteen experimental designs (3 pre-experimental, 3 "true", and 10 quasi-experimental) and some variations on them have been presented.

The check-list of validity factors (Tables 1, 2, & 3 in reference texts) is an important aid in interpreting the results of such experiments.

NOTE: Almost all credit for the above must go to Campbell and Stanley's chapter in N. L. Gage's Handbook of Research on Teaching, from which the "plagiarized" material was freely taken.

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